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The morals of model-making

S.G. Sterrett [a,b]

[a] Department of Philosophy, 135 Baker Hall, Carnegie Mellon University, Pittsburgh, PA 15217, USA
[b] Department of Philosophy, Wichita State University, 1845 North Fairmount, Campus Box 74, Wichita, KS 67260-0074, USA

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ABSTRACT

I address questions about values in model-making in engineering, specifically: Might the role of values be attributable solely to interests involved in specifying and using the model? Selected examples illustrate the surprisingly wide variety of things one must take into account in the model-making itself. The notions of *system* (as used in engineering thermodynamics), and *physically similar systems* (as used in the physical sciences) are important and powerful in determining what is relevant to an engineering model. Another example (windfarms) illustrates how an idea to completely re-characterize, or reframe, an engineering problem arose during model-making.

I employ a qualitative analogue of the notion of physically similar systems. Historical cases can thus be drawn upon; I illustrate with a comparison between a geoengineering proposal to inject, or spray, sulfate aerosols, and two different historical cases involving the spraying of DDT (fire ant eradication; malaria eradication). The current geoengineering proposal is seen to be like the disastrous and counterproductive case, and unlike the successful case, of the spraying of DDT. I conclude by explaining my view that *model-making in science* is analogous to *moral perception in action*, drawing on a view in moral theory that has come to be called moral particularism.

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1. Introduction

That the practice of science involves values and norms is no longer in question. At least, my starting point here is that that question is already settled. The question can be pushed farther down, though, to the level of specific activities involved in scientific practice. The question concerning the role of values and norms then arises for one of the most ubiquitous activities in the practice of science: *model-making*. Does it make sense to ask questions about the role of values and norms for such technical aspects of the practice of science? Assuming that it does, we can go on to ask: Are there some such activities for which values and/or norms are *especially* or more intimately involved? At the other end of the spectrum, are there any such activities that do not involve values at all?

For this symposium, I thought through these questions for model-making,[1] and, as a result, came to see model-making as, in some cases, akin to moral perception; to put it more precisely, I came to see *model-making in science* as analogous to *moral perception in action* on the account of it given by the philosophical view known as moral or ethical particularism.[2] In this paper, I will try to explain how I came to this view.

2. Model-making: impact and nature

There are reasons to suspect at the outset that model-making might involve values and norms. For, model-making is employed in *describing* and *conceiving* and, consequently, might be expected to have an impact on actions taken. Common sense and critical examination of life experiences are probably sufficient to indicate that this is indeed the case, but there is also some experimental research in the behavioral sciences that illustrates the existence of what have become known as *framing effects*: the phenomenon that

E-mail address: susangsterrett@gmail.com

[1] For this invited talk, I was asked to specifically address the topic of models in engineering science.
[2] An introduction and overview of moral particularism is given in Dancy (2009).

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people choose differently depending upon the descriptions used to present the choices available to them. Experiments designed to exhibit such effects have been carried out for groups reaching a group decision, as well as for individuals making a choice. Instructed to indicate their choice, which option people say they prefer is sensitive to *how the options presented* to them are parsed and described; it is sensitive to *how situations are characterized* in the experiment, it is sensitive to *the consequences that are identified* in presenting the issue in the experiment, and, even, to *the perspective (individual, group) from which the consequences are described.*[3] How things are conceived of, evidently, makes a difference to what people would choose, at least within the constraints of, and when confined within, the laboratory.[4] Of course, how individuals act outside the laboratory may well differ in some ways from the behavior they exhibit in the laboratory situation set up in order to make predictions, so caution is in order when predicting actual behavior based on such experiments. Nevertheless, these experiments are informative on more general points. The general point I take from this experimental work is that they confirm that *how an agent responds to a situation can depend upon how the agent conceives of that situation.* As models are means of conceiving things (entities, situations, processes, etc.) model-making can, accordingly, have an impact on what agents do. That models have such consequences indicates that it is unlikely that model-making can be decoupled from values.

Other hints that model-making might involve values or norms come from the practical experience of those who have made models. It is part and parcel of model-making, whatever the field, and for almost every kind of model, that one must make choices about what the model is to include. There is generally going to be more than one way to model a given thing, situation, or process. This simple fact raises the question of what kinds of constraints or norms, if any, are employed to winnow down or rank the different ways one could make the model.

Yet, one could ask, even if values and norms are involved in the *use* of models, does this fact alone really determine the answer to the question as to whether model-making necessarily involves values and/or norms? On reflection: no, it doesn't. We might try distinguishing between what's involved in the model-making from what's involved in using the model. Once we do, we see that the fact that values and norms are involved in the *use* of models alone doesn't rule out the possibility that the model-making activity itself could be decoupled from specifications about what the model is to accomplish. Specifications for the model could be developed with the use to which the model is to be put in mind. So, decoupling looks possible. In fact, some might find it quite natural to wonder if it is not the case that *all* values and norms associated with the use of models are due to interests or values that can be identified either *before* the model-making occurs (i.e., in the specification of the problem that the model is made to help deal with) or *after* the model-making is complete (i.e., in using the model as a guide to what actions one will take).

To help gain some clarity on this, let us look more carefully at just what is involved in model-making. I'll begin by first examining models of some relatively circumscribed targets: models of machines. Then, I'll look at examples of models constructed for use in engineering the environment. Finally, I'll look at some proposals for engineering the planet.

3. An engineering model of a machine is never a model of *just* a machine

Since we are interested in examining the objection that the activity of model-making might be separable from interests, and that the source of values and norms that show up in the course of model-making are all attributable to the interests involved in either the specification or use of the model, let us begin by looking at cases towards the end of the spectrum where model-making is most circumscribed.

Consider the following example: making a full-scale reproduction, i.e., replica, of the original 1903 Wright Flyer.[5] This is a model-making task in which the target is relatively well defined and the goal of the model is defined sufficiently clearly that choices about how the model is to be made are relatively constrained by the problem definition. To draw out the point I want to make here about *engineering models* (versus *physical replicas*), let's clearly distinguish two different problem definitions.

Problem R (Make a Physical Replica of a Machine): Produce a physical object that is as close as possible to being exactly like the physical object that existed in 1903 now known as "The 1903 Wright Flyer" in terms of the physical properties it had when it was built and flown in 1903.

Problem E (Make an Engineering Model of a Machine): Produce a physical setup that allows one to determine the dynamic behavior (e.g., the forces, deflections, and motions) of the 1903 Wright Flyer during the flights that were made in it in 1903.

Neither problem is unusual, and each involves modeling with historical accuracy (i.e., one of them is to model a physical object that actually existed at one time; the other is to model behavior (position, momentum, forces) during an event that actually took place).

Different interests sometimes call for different problem characterizations, even for the same event or topic under investigation: Problem R is the appropriate problem statement, were someone to commission a work for a museum aimed at exhibiting a certain machine that once existed, with historical accuracy. Problem E is appropriate were someone to want to recreate selected scientifically-relevant aspects of the events in the historical record that the original researchers would have experienced when carrying out the experiments their notebooks indicated they carried out with that same physical machine. It is easy to conflate Problem R and Problem E.

Are these *necessarily* different model-making problems? Couldn't one model satisfy both of the problems set? The answer is that even if the same physical object that constitutes a solution to Problem R can be *used in* a solution to Problem E, the problems are really quite different and the models meeting those problem specifications are not comparable. So the answer is that they are in fact different model-making problems.

Perhaps some explanation is in order here. Since an engineering model is concerned with behavior, anything relevant to behavior is part of the model specification. The behavior of interest in Problem E (the engineering model) is similarity of processes, in that the model is to produce, possibly after rescaling, the same forces, deflections, and motions as the historical event. The behavior of interest in Problem R (the reproduction, or replica model) is to make a working replica. Now, one might think that making a working replica just is making a model that behaves the same way, but

[3] There is a large literature on framing effects; a recent review is Stalans (2012). Paese, Bieser, & Tubbs (1993) discuss the relation between individual and group framing effects. Druckman (2001) proposes a method of evaluating the strength of framing effects due to a particular frame.

[4] The design and interpretation of many of the experimental studies meant to present particular instances of framing effects and establish claims about the mechanisms at work have sometimes been rightly criticized. However, I think the fact that framing effects exist and are frequently operative is well established. That is all I am relying upon here.

[5] Also discussed in Sterrett (2006).

this is because what is suppressed in such reasoning is the relationship that exists between the larger system that determines the physical replica's behavior, and the physical replica itself. That relationship is crucial; to leave it out is to specify only part of the model. To explain: engineering models aim at similarity of a certain specified physical behavior or response. Thus engineering models must include all the things that determine that physical behavior or response—even if only as variables that take on different variables for different cases.

(For readers to whom this point is still unclear, perhaps the following analogy may be helpful: If the specified physical behavior that one is interested in modeling depends upon something that is not included in the model, making the model that way is as though one answered a question by producing a string of words that would make a sentence if only the right kind of verb were placed at just the right place. To justify giving such a response to a question by saying, "Well, the ordered string of words gives the same result as the correct response if the same word is added to it at exactly the same spot as it occurs in the correct response, so my response really behaves the same way as the correct answer does in each case—and therefore it really is the same sentence as the correct response" is to misunderstand what a reply to a question is supposed to do. Even if one wishes to give a reply in which a verb is to be left unspecified due to wanting to cover a multiplicity of cases, simply leaving the verb out does not indicate that the nature of the reply depends upon a verb, nor how it does so.)

What if one were to define a new problem that encompasses both Problem R and Problem E as specified above? Something like that approach was taken by the project called The Wright Experience, and here is how it actually proceeded: The project was inspired by the conviction that "The best way to rediscover how the Wrights accomplished [their] incredible achievement in so short a time is to experience ourselves what the Wrights experienced." The scope and depth of the model-making project was astounding. In the actual project that was carried out, the attempt at replication was so thorough that period tools were made so that the replica machine would be constructed using tools of the same size, materials, and construction as the tools used in constructing the original. Testing was carried out using both wind tunnel and flight tests.[6] The human-machine interaction aspect was thoroughly studied and replicated as well: the same pieces of equipment that the Wright Brothers had practiced on prior to the first trial of their 1903 Flyer were also painstakingly replicated and tested, so that their twenty-first century counterparts could learn about and train on the same machines as those who had flown the machine in 1903 had.

One of the replicas was built to be flown in celebration of and on the 100th anniversary of a flight in 1903 made by the original 1903 Wright Flyer. The 100 year difference in time certainly shouldn't matter to how the machine functions. Yet, when the time finally came to launch it on that day in 2003, it did not fly as it had on the day in 1903 one hundred years before. Why the difference, if the model of the plane was practically a perfect replica, the human interaction as closely replicated as possible, and the location about the same? Simple: the weather was very different on the two days (the one on 17 December 1903 and the one on 17 December 2003), and the replica, of course, replicated only the machine and the manipulation of its moving parts controlled by the pilot. The scope of the replica model (Problem R) did not, of course, include the surrounding atmosphere; a proper specification of its behavior in flight would, of course, include a characterization of it that included all the features of the atmosphere that determined the machine's behavior that was of interest. Thus the model-making activity associated with Problem R did not distinguish between situations that would yield flying behavior similar to the original 1903 event, and those that would not.

As a replica of a physical object to exhibit as a museum piece, the replica model may well be an excellent example of model-making. The replica machine was even presented in a replica of its scientific context, in that it was flown by pilots trained on replica or vintage flyers of the era, and was accompanied by the presentation of a great deal of information about how the inventors' thinking about the machine evolved as they developed and tested prototypes in succession, up to and including the 1903 Flyer. However, *as a model of a replica of a functioning flying machine*, the physical replica of a machine, as sophisticated as this one was, is an inappropriate notion of model. The scope of the model of the replica machine would need to be expanded in order to make it an appropriate model of a functioning flying machine.

Just how would one expand the scope of the model to include the surrounding atmospheric conditions, and in a way that ensured the flying behavior would be similar to the original 1903 event? What the model-maker would need to be able to do is establish criteria for physically similar situations. This is not only possible, but the methodology for this kind of case is relatively well established; the kind of similarity that matters to flying behavior is dynamic similarity.[7] Often dynamic similarity cannot be achieved by any model except a full-size one; but this replica was a full-size model, so such an analysis might have been possible; instead of choosing to hold the event on the same day of the year, it could have been held when atmospheric conditions were similar to the event in 1903 in the ways specified by the engineering model. The way that the atmospheric conditions figure in the model-making is via the *criteria for dynamic similarity*. These criteria are expressed in terms of certain ratios of quantities that one aims to make invariant between the model and the target situation. The ratios will involve measurable quantities having to do with atmospheric conditions (e.g., pressure, temperature, and humidity) as well as quantities having to do with the physical features of the aircraft. That is, the criteria of similarity are that the important relationships holding in the original event also hold in the replica constructed by the model-makers. The model-makers recognized this, in a way—reporting on the event in retrospect, they point out that they re-enacted a different event than the one the 2003 flight was meant to commemorate: "Our attempt at flight on December 17, 2003 replicated almost exactly the Wright brothers' first public attempt at flight, May 23, 1904. Lack of wind, engine trouble, and Wilbur got as far as the end of the rail…and went nowhere."[8]

This point about the scope of a model is not special to the field of flying machines. I find the building of the replica of the 1903 Flyer a striking example of a more general point. Put informally: *An engineering model of a machine is never a model of just the machine.*

Consider model-making of the classic simple machines, which may at first seem to be paradigmatic examples of models of just a machine, e.g., the pendulum, the spring, or the lever. The pendulum is anchored, though, as is the spring, and the lever must be provided with a fulcrum of some sort. If we consider simple models such as the billiard ball model of a given volume of gas molecules, we see that it is crucial that the billiard balls be in a

[6] According to The Wright Experience (2012).

[7] I provide a discussion and more detail on the definition of dynamic similarity in my other works, especially Sterrett (2002, 2006, 2010), and in Chapters 6 and 7 of *Wittgenstein Flies A Kite: A Story of Models of Wings and Models of the World* (Sterrett, 2005). Basically, whereas *kinematic similarity* ensures that the motions of the model and target systems are geometrically similar (i.e., that ratios of distances traveled and ratios of velocities in the target are preserved in the model), *dynamic similarity* ensures that the forces between the model and target systems are similar (i.e., that ratios of forces are preserved).

[8] "The Flyer in Flight—2003." The Wright Experience (2004).

constrained space in order to model the gas molecules that are constrained by the walls of a container. What about things for which the system boundaries can be chosen, and so represent merely conceptual boundaries, such as the control volume concept in thermodynamics? It may seem at first as though the idea of a control volume liberates us from having to consider the surroundings of a system we wish to study, since, once the boundaries around the system are chosen, all one need consider is what crosses those boundaries. The twist is in how the boundary must be chosen if one is to have a good model of a system one wishes to analyze. Misconceptions about the leeway a model-maker has in choosing system boundaries are all too common. Often people think of the boundary of a control volume or a system as a surface like the wall of a balloon, i.e., that the value of a property on one side of the boundary differs from the value on the other side, with the boundary serving to demarcate a discontinuity. But nothing could be further from the truth when it comes to *correctly* choosing a system boundary of a system for engineering analysis. Adrian Bejan explains what's involved in properly selecting the boundary between a system and its environment:

> "To define the system means also to identify sharply the system's environment, or surroundings. The environment is the portion of matter or region in space that resides outside the system selected for analysis. What differentiates between the system and its environment is the surface called boundary . . . the boundary is a surface, not another system . . . the value of a property that is measured at a point on the surface called boundary must be shared by both the system and the environment because, after all, the system and the environment are in contact at that point." (Bejan, 2006, pp. 1–3)

This clarification of how an engineering system boundary must be defined is really a more general way of expressing the points made earlier about models of simple machines and systems such as the pendulum, spring, lever, and ideal gas in a chamber. It shows why, I think, we cannot in general expect to be able to understand or explain physical behavior by mentally decoupling engineering systems from their environments, i.e., by making a model of "just the machine." It's because *system boundaries don't allow one to chop the engineering system off from the rest of the world for analysis; they make sure the role of the environment is included in the model so that one can do the analysis correctly*.

I want to emphasize that the point just stated about system boundaries holds even for the kind of systems known as closed systems in thermodynamics. To explain: a distinction is made in engineering between *closed systems* and *open systems*: "A system defined by a boundary impermeable to mass flow is a *closed system* . . . systems whose defining boundaries can be crossed by the flow of mass are *open systems*, or *flow systems*." (Bejan, p. 3) Flow systems are qualitatively different than closed systems, to be sure, and they deserve a whole field of study of their own. Nevertheless, because there can be energy exchanges across the boundary of a closed system, the point just made in the previous paragraph about the boundary of a system ensuring that the role of the environment is appropriately included in the definition of the system applies to closed systems as well as to open systems.

4. Engineering design & human behavior: how far must the net be cast?

So far we have said little about the humans in the surroundings of the machines being modeled. If we are discussing a machine such as a lever or a hammer, a human might be involved via mechanical contact with the machine by touching or grasping it, say—much the same way that, for a pendulum, an inanimate object in its surroundings would be involved in mechanical contact with the pendulum (e.g., if the pendulum was supported by an anchored hook, say). Thus the study of human-machine interaction is important in a proper analysis of many machines, even from a purely mechanical standpoint.

If we stick to the purely mechanical standpoint, what difference does it make if there are humans or not, from the point of view of performing an engineering analysis? It might seem that during the model-making, the role of the humans in the environment will be simply whatever role they have in virtue of the mechanical or physical effects that their presence and actions have. This may well be true, but even if it is, it leaves unanswered another question having to do with identifying the engineering system that is to be analyzed: how far out do we have to look to make sure that we have gone far enough to include all the relevant factors in the analysis of an engineering system? That is, suppose we have correctly identified the boundary of our system. We now know what the points of contact between our system and its surrounding are. The question we are faced with at this point is: What do we need to know in order to be able to properly characterize everything that we should take into account at these points of contact?

The same questions arise for writing specifications in design engineering, except that there they take the form of asking how far one must extend the net in order to capture all the factors that must be specified as part of the model of that machine, in order to ensure that it performs the function for which it was designed. For an engineered object, these questions arise both when we are modeling the envisioned object in order to write proper design specifications for it, and (all too often) afterwards, when it is already in use and we need to troubleshoot or answer new questions about its behavior. A particular design problem of general significance that is well-suited to illustrating the predicament that a model-maker faces is the design of nuclear power plant containment sump screens.

First, a few facts about nuclear power plants to set the stage for a discussion about containment sump screens: nuclear power plants are designed such that, for certain kinds of accidents, the containment (the structure in which the nuclear reactor is housed) is isolated from the rest of the plant, in the sense that flows into and out of the containment are cut off.[9] There are a few exceptions, e.g., venting operations; and incoming flows that provide water needed for accident mitigation to ensure reactor safety. Water is injected into the reactor, and, since there is no way for it to leave the containment after the containment is isolated, any water that leaks, is ejected, or otherwise spills out of the reactor system stays in the containment building.[10] At some point, no more additional water is to be injected into the reactor (you wouldn't want to keep adding water to the containment indefinitely; since flowpaths out of the containment are all isolated, the containment would fill up). At this point, the emergency safety pumps stop injecting new water. Instead, the water that is already inside the containment is pumped to the reactor core—it will by this time have collected in the sump area, so as to be ready at the suction of the emergency safety system pumps when needed.

In terms of basic physical configuration, containment sump screens are quite simple. They are just screens. Their function is pretty straightforward, too: to screen debris in order to prevent it from entering the safety pumps, which are critical to reactor safety. During this phase, they suck fluid from the sump area of

[9] U. S. Nuclear Regulatory Commission. "Containment Isolation Provisions for Fluid Systems. Regulatory Guide 1.141." April 1978.

[10] "Description of the Safety Concern; PWR Sump Performance." nrc.gov. Nuclear Regulatory Commission, (2011) Web, downloaded 28 March 2013. This webpage features a schematic showing the role of the sump screen pre- and post-accident operation.

the containment building and inject it into the reactor to remove the decay heat from the fuel. As to the question of enumerating all that could affect the ability of the screen to perform its intended function, the answer is not straightforward. The question of how far the net must be cast, so to speak, arises. Thus, it turns out that, if we ask how one ought to *specify* the important features of its design, the answer is not at all definite. What counts as part of the design has been a matter of contention, even among experts with experience. Everyone agrees that the screen is needed in order to keep debris from entering the pump suction: that's the reason for its existence and location. Everyone agrees that if too much material of whatever sort has collected on the screen at some earlier point in time, it can cause a lowering of pressure as the flow passes through the gunked-up screen at any later point in time. This is a serious problem: the pump needs the pressure on its suction side to be above a certain threshold. There's the rub: the same containment sump screen that protects the safety pump and everything downstream of it (including the reactor core) by preventing debris from entering it can present a different danger to the very pump it was designed to protect: should the screen become clogged, the pump's suction pressure might be lowered to a point where there is danger of pump failure.

The contentious issue of what should be considered a design feature of the sump screen has to do with the range of things that could impact the ability of the screen to perform its intended function (screening debris but permitting fluid flow), for the range is expansive and includes a lot of things in the plant that are otherwise unrelated to each other. The usual things one takes into account in designing a screen are features of the screen itself, such as hole size, strength, ductility, corrosiveness of the materials, resistance to seismic disturbance, and so on. But, after the plants were in operation, some of the unusual events observed at plants showed that another likely source of blockage was due to a source not considered in the original design conception of what a screen ought to do. Some of the reduction in flow through the screen was due, not to large objects that could be kept out by appropriate choice of hole size, but due to fibers and sludge buildup that happened to be in the containment and got into the water when water was sprayed or delivered by the emergency safety systems inside the containment during an accident. The fibers and sludge buildup then make their way, along with the draining water, to the containment sump area. Many different kinds of objects: cleaning cloths left in various areas; paint flecks coming off during the accident due to the severe conditions inside the containment; packing or insulation becoming dislodged from some other piece of equipment in the plant unrelated to the safety pumps; and so on, can come to rest on the containment sump screen during the later, recirculation phase of the accident and clog it up. Under the temperatures and pressures possible during an accident, chemical reactions can take place making things even worse for the sump screen performance. Too much of this kind of debris caught on the screen can result in such a low pressure at the pump suction that the pump can't operate properly, and can even damage itself trying. Since the pump has to function during nuclear power plant emergencies, stopping to clean or change out the sump screen is not an option. The sump screen is on its own during an accident, so to speak; there is no contingency plan possible. It has to perform its function without causing ruin of the emergency safety system pumps.

Thus begins the casting of the net to try to catch all the factors relevant to the containment sump screen performance. What must be included in the model of the screen the model-maker constructs and uses in order to specify the design features of the containment sump screen? The design features are those that must obtain so that the containment sump screen does the job of collecting debris but does so in a way that does not impair pump performance. It is only in the process of this model-making that the model-maker becomes slowly aware that specifying the amount and kind of things that could reduce flow through the screen, including things that are not usually thought of as debris or loose objects, is part of the design of the screen itself!

The model-making of the screen, an item so simple in physical configuration (i.e., an extremely simple machine), then gets extremely complicated. How can the enforcement of cleanliness standards and practices in parts of the building far away from the screen be incorporated into the model and/or the design specifications of the screen? It then becomes obvious that social structures and administrative effectiveness are as crucial to how well the containment sump screen performs during an accident as equipment structures are, and that how reliably a plant's cleanliness rules are enforced is likewise as relevant to making a model of how things will go in an accident as the reliability of its pumps is.

The point generalizes: model-making in engineering analysis has to take into account the nature and structure of the administrative and social organizations that affect the surroundings of the machine or system. What looks at first like a straightforward matter of model-making in engineering design and analysis turns out to be a matter of far wider scope. That scope includes things that seem at first to have little to do with something as simple as the design of a screen, things that have to do not only with features of other equipment in the plant otherwise quite unrelated to the system of which the screen is a part, but that have to do with rules about mundane day to day human activities throughout the entire life of the plant seemingly unrelated to accident response, such as cleanliness standards for the containment building.

5. Model-making and scope

From the two examples used so far (a replica of an early flying machine and a sump screen in a nuclear power plant), we've seen that, if one is to do it well, the activity of model-making has to involve looking beyond the physical boundaries of the item being modeled. But, other than the discussion of how to select the boundary for an engineering analysis properly, so far we've merely looked at examples of how wide a scope might be needed. Is there a methodology to follow to ensure one looks in all the right places? Is there a criterion one can use to determine if one has looked far enough?

Well, we know that there is a criterion for physical behavior: the criterion of physically similar systems. The criterion for two systems to be physically similar systems can be stated generally, formally, and precisely: First, find a convenient set of dimensionless parameters that characterizes the behavior of interest (e.g., buckling, heat dissipation, patterns of flow in porous media). This is not straightforward, and can involve experimentation and trial and error. The import of achieving this step is that two systems will be behaviorally similar just in case the value of the dimensionless parameters are the same in one system as in the other. The dimensionless parameters are ratios and thus pure numbers.[11] However, identifying a criterion and applying a criterion are two different things. The criterion, though, can be stated with precision.

Applying the criterion of a certain kind of physical similarity to a particular model-making problem, however, requires drawing on

[11] The dimensionless parameters characterizing the system behavior of interest are often thought of as ratios of some meaningful quantities (e.g., a ratio of lengths for geometrical similarity; a ratio of velocities for kinematic similarity; a ratio of forces, for dynamic similarity) as a help in cognizing and reasoning about the two situations, though it is not necessary to do so to use the criterion.

knowledge specific to the type of problem that the model is being built to address. This is not too surprising, once we reflect on the status of the principle behind recognizing and establishing similarity of systems. It is the logic of analogy spelled out for the case of reasoning about physical behavior. Its status is that of a logical principle; one might say that it is the logic of reasoning about physical behavior. The criterion of physically similar systems just stated is a more general version of the criterion referred to in the discussion of the replica flying machine. There, the criterion that the model of the system be physically similar to its target was the requirement of dynamic similarity between an event in which the replica model was flown, and the original event in which the plane of which it was a replica was flown. The tricky part of the logic of analogy (which system features should be the same in the two situations between which the analogy is drawn?) is dealt with by this criterion. Or, rather, it is reduced to a simpler question: if one knows which quantities are involved in the relations governing the behavior of interest, one can identify the dimensionless quantities (ratios) that need to be the same in the model as in target in order to be able to set up a correspondence between the quantities in the model and the quantities in the target. This question is simpler to answer, for all that is being asked is which physical quantities the physical behavior depends upon—not what the dependence is nor what must remain invariant in order to preserve the same behavior. The more fundamental question of which quantities the behavior depends upon is not a matter of logic, however. This is true even though the criterion can be stated in a logical formalism.

Hence, in practice, although the criterion for (physically) similar systems is indispensible in organizing one's model-making efforts, when used in conjunction with the points made earlier about properly defining the boundaries of models used in engineering system analysis, being in possession of the criterion for similar systems does not obviate the need for experience and experimentation in determining which quantities are relevant to characterizing the system.

I have written extensively in other works on the history and the philosophical significance of the notion of physically similar systems for systems characterized by quantitative relations. (Sterrett, 2002, 2005, 2006) I believe the notion of physically similar systems is a specific case of a more general logical criterion of what we mean for one thing to be a model of another, even for non-quantitative features of a situation. How might the notion of similarity be stated for similarity in cases where we are interested in relations, but the relations are understood and characterized *qualitatively* rather than quantitatively, as ratios? I do not see why we cannot appeal here to the notion of similarity that motivated development of the formal criterion in the quantitative case: *What makes a good model good is that the behavior (of interest) of the model is similar to the behavior (of interest) of what is modeled. The criterion that ensures that the behavior is similar is that the relations from which that behavior follows in what is modeled be identified, and then shown to also hold in the model.*

"Behavior" here can mean different things, and the criterion of similarity of systems differs accordingly, depending on which particular behavior of the target one wishes to model. (The difference between kinematic similarity and dynamic similarity was mentioned above, i.e., if the behavior that one wished to model was the motions of items in the system but not necessarily the forces on them, the kind of similarity the model ought to have to the target would be *kinematic similarity*; if, in contrast, the behavior of interest was the forces produced, then the kind of similarity between the model and target to aim for would be *dynamic similarity*.) I believe that the criterion provides some guidance in model-making for cases where the behavior of interest is qualitative; the idea captured is that of being analogous with respect to a qualitative feature. Thus, something like *a good x* or *an effective y*, e.g., a good *response to an opportunity*, or an effective *means of stimulating growth*.

Once this has been settled, the next step is to properly conceive of what it is that is to be modeled. One way in which the criterion provides guidance during model-making is that, in order to apply it, the model-maker must define a system, in a precise sense of system (i.e., the state of the system can be characterized in terms of a set of quantities; any or all of the quantities may vary over time, but whatever their value at a certain point in time, we consider the values of them jointly to pick out a system state). For a non-quantitative example, consider a qualitative feature of a certain area of a city such as saying that the traffic is flowing smoothly, versus that it is slow or jammed. We might want to characterize such a feature in terms of what we would see at a snapshot in time (car locations and speeds), or we might wish to characterize it in terms of some more abstract quantity having to do with delays people experience. There may be many different combinations of vehicles at different locations going different speeds that would constitute one of these states. Yet, the state (smoothly flowing traffic) is determined by the locations and speeds of the cars. So, after clarifying the behavior of interest, it is crucial to identify a system, namely, *the system* that *determines* the *behavior of interest*. To understand why this is a nontrivial requirement, consider how the *systems* in the examples discussed (replica flyer, sump screen) had to be defined in order to meet this requirement properly. The main point made about proper selection of a system boundary (i.e., no discontinuities of relevant quantities at a boundary) then applies.

We have seen that the scope of a model can, and often does, include the influence of features of social organizations. When this does happen, then, as in the sump screen example, interests and values are identified during the model-making itself. But something else can happen in the process of model-making. The person doing the model-making is doing so in the service of modeling a given target with respect to a certain behavior of interest. As we might expect, the detailed model-making can provide feedback about how the behavior of interest depends on various features of the model. But, occasionally, the person carrying out the model-making also gains some insight about the model, the target, and the behavior of interest that makes him or her question the suitability, not of the model, but of the target. The model-making points out a certain feature of the target, and questioning the suitability of the target for solving the larger engineering problem that occasioned interest in pursuing the problem in the first place can lead to a complete overhaul of the conception of the problem. How common or rare this is is not our concern here—that it can and does occur at times is germane to our question about values and norms in model-making. We turn now to an important example in which this really happened.

6. Better targets

The example in which the question of the suitability of the target of a model arose in the course of model-making itself concerned the problem of developing an improved windmill to transform mechanical energy in the wind into electrical energy. The usual way of approaching the problem is to choose the windmill as the engineering system to be analyzed, and to model different possible windmill designs (e.g., to compare a *horizontal-axis* style wind turbine (HAWT), in which the blade is mounted like a pinwheel on its support, with a *vertical-axis* style wind turbine (VAWT), in which the blade rotates in the same direction as a playground merry-go-round does).

Suppose we ask: what are the interests to be addressed in the model-making when wind turbine efficiency is the target? The list would include things such as: accuracy (in the values of efficiency), and the inclusion of uncertainty ranges when those values are reported. We want the model-making to be thorough in considering kinds of locales, the height of the windmill tower, the range of possible sizes and styles of blades. The study should also identify and quantify the impacts on the environment and wildlife, as well as the socio-economic impacts on humans, throughout its entire lifespan. We'd want our model-making to use a wide range of weather conditions based on up-to-date information and, even, on future climate projections. So far, we see no problem in capturing the important interests in the problem specification; all these goals can be incorporated into the model-making activity.

That is, in fact, how much of the engineering modeling approach to land-based wind turbines looked for a long time. But, in light of recent work by a research group in engineering and applied science directed by John O. Dabiri, even addressing all these interests might not be aiming one's efforts at the best target.

Dabiri explains what's different about his current research in wind energy: The usual starting point, he says, has been to focus solely on the *wind turbine efficiency*. (Dabiri, 2011) When efficiency of an individual wind turbine is what is identified as the behavior of interest in making the model, and the model's target is a single windmill transforming mechanical energy of the wind to electrical energy, the model-making enterprise focuses on the answer to the question: "What fraction of wind energy flux through the swept area [of the rotating turbine blades] is converted to electricity?" Comparing a single horizontal-axis wind turbine (HAWT) with a single vertical-axis wind turbine (VAWT), the result is clear: there are HAWTs that are more efficient than any VAWT, and they can be built. This approach is very common in the design of electrical generation machinery: i.e., first, *the most efficient type* of a certain kind of machine is determined (here, a HAWT), after which *the maximum theoretically possible efficiency* for the type of machinery that was deemed to be the most efficient type (here, a HAWT) is then determined. The maximum theoretically possible efficiency is then used as input to a calculation performed to provide an *upper limit* to *the maximum amount of wind power that can be extracted from a wind resource* (amount of power per unit of land area). For HAWTs, this value is just under 60%, and is known by the name "Betz Limit" (short for Lanchester–Betz–Joukowsky Limit). As there has been a lot of work directed towards maximizing the efficiency of wind turbines, the current designs are not that far away from the Betz limit and increasing the speed doesn't improve it further. Thus it doesn't seem that there is much more that could be done. The Betz Limit was regarded as a rigid limit and the other parameters associated with optimized HAWTs were used to estimate how much of the earth's energy could be harvested by terrestrial wind farms.

Dabiri's thoughts turned to *the target of the model*, rather than to the model of the target. Instead of making *turbine efficiency* the behavior of interest, he suggested, shouldn't we instead make *wind resource utilization* the behavior of interest? (Dabiri, 2011) Correspondingly, he suggested, we should replace the old question "What fraction of the wind energy flux *through the swept area* of a wind turbine is converted into electricity?" with the new question "What fraction of *wind energy flux into the wind farm volume* is converted to electricity?" Thinking in terms of the entire wind farm as the target to be analyzed meant that his model could now include parameters that models of the old target did not include: spacing of windmills and direction of rotation of the windmills. The target of the model had to change, it seems, in order to be able to include these parameters in the model.

Before, it seemed, the Betz Limit on a single turbine, multiplied by the number of (suitably spaced) turbines that could be placed on a certain expanse of land, would yield the total amount of electricity that could possibly be generated from a certain land area. But on the new approach, with this new problem specification, alternative ways of seeing the potential in the same land area could arise. Looking at the situation from the standpoint of the new question, one could ask about the unharvested wind energy between windmills. On the old approach, the optimized HAWTs had to be spaced far apart so that the wake of one windmill would not impair the performance of another, which meant that there was wind power not being utilized between and behind the optimized HAWTs in the usual wind farm layout. (Dabiri, 2011)

Nature often finds optimal solutions, or at least tends towards them. How did nature deal with similar situations? Dabiri's research group saw an analogy between wind farms and schools of swimming animals that propel themselves by tails or other appendages. (Whittlesey, Liska, & Dabiri, 2010) They noticed that the swimming animals did *not* need to stay far apart from each other. And they noticed something else about them: they formed a pattern of motion in which the swimmers did not all move in the same exact way, but whose motions complemented each other. Using the analogy in the direction from nature back to his problem of wind farm design, he saw that the analogous approach on a wind farm would be to use VAWTs that were a good deal smaller than the large size that HAWTs needed to be to maximize their efficiency, locate these small VAWTs closer together, and let them take advantage of each others' wakes, by having each VAWT rotate in the direction *opposite* to its neighboring VAWT. What I describe informally here is stated more precisely in the published works of Dabiri and his colleagues. They tested the biology-inspired design of wind farm layout; their pilot studies indicate that it may well turn out to be that vertical and smaller wind turbines can be placed much closer together than HAWTs, and that a windfarm of smaller VAWTs can actually be more efficient than a windfarm of large HAWTs—*in spite of the fact that* an optimized single HAWT is more efficient that any single VAWT. (Dabiri, 2011)

There are many logistical, social and environmental benefits of smaller VAWTs, too. To be most efficient, HAWTs have to be extremely large—an order of magnitude larger. As a consequence, HAWTs of the size called for by optimizing on a single windmill basis pose logistical and maintenance problems (the blade alone is unwieldy to transport)—not to mention the concerns about environmental hazards to wildlife. Because of their scale, the operation of these very large windmills is disruptive to their immediate surroundings, and, some even feel, can render the regions in which they are located unsuitable for habitation by humans and some wildlife.[12] Whether or not farms of VAWTs are shown definitively to produce more electricity per land area, the lesson is that one must distinguish between different ways in which a problem of energy resource use is approached, for the use of a different approach can have radically different results. It now makes no sense to ask simply what the environmental effects of wind power are; a windfarm of small VAWTs looks to be far more benign environmentally than one of tall HAWTs.

The point of the example about thinking in terms of wind*farms* instead of in terms of wind*mills* for the question in this paper is that interests can be clarified—*and might even be reformulated—during* model-making. It is worth emphasizing that, given that the behavior of interest in the old problem was more narrowly cir-

[12] A recent documentary (Windfall, 2012) gives voice to concerns residents near current and proposed large HAWTs have expressed about this, and, in the case of a current windfarm, gives an account of the social disruption that followed upon construction of windmills on existing farms.

cumscribed as wind turbine efficiency, the model-making that selected horizontal axis wind turbines (HAWTs) as better was not a matter of erring. It is not that those earlier researchers erred in how they carried out their project; in fact, it was expertly executed given the circumscribed problem statement. What Dabiri realized was that there was *another behavior of interest that provided a different target to be modeled, one that better addressed the genuine interests of those interested in wind energy*. That realization was spurred by the similarity he recognized between an actual group of swimming creatures moving together in each other's wake in the schools of fish he observed in nature, and the vision it suggested of how an entire windfarm of wind turbines might be arranged. (Whittlesey et al., 2010)

7. Using similarity to suggest and evaluate solutions

Model-making is always carried out with respect to a behavior of interest. In a rational reconstruction of a model-making activity, the behavior of interest is specified first. In reality, things are often more like the windfarm example: there are subtleties about the behavior of interest that are clarified *in the course of* model-making. How should this critical attitude towards the identification of a behavior of interest—i.e., the step wherein it is suggested that the *wind resource utilization of a windfarm* replace the efficiency *of a wind turbine* as the behavior of interest—be described? I believe it can be very difficult for someone making such a suggestion to get this point across, because the behavior of interest is often put too generally. In the wind example, someone already convinced that the use of HAWTs is dictated by the fact that an HAWT can be made to be more efficient than a VAWT might not see that the difference between the more general specification: "harvesting the most energy from the wind" and the more particular specification: "maximizing wind turbine efficiency" could make all the difference in how the model-making will proceed.

What point ought we to draw from the windfarm example? It might at first appear that the point to draw is that a modeling problem must be specified in more general, rather than more particular, terms, so as not to bias the model-making in a way that precludes getting the best solution to a problem. However, notice that, when characterized in more general terms ("harvesting the most energy from the wind"), the modeling problem becomes much less determinate; when specified in these more general terms, the first step to take in modeling the situation in order to determine the best technological solution is far less clear. When a problem is specified in more general terms, the activity of model-making requires much more of the specification to be done by the modeler; generalizing the problem is not really going to help direct the solution. In the example described above, what directed the solution was the modeler's use of his creative intellect and imagination in discerning the similarity between a fish propelling itself through the ocean by the motion of its tail and a windmill harvesting the energy in the motion of the wind. Then, he thought to pay attention to how the fish moved with respect to each other when traveling in the ocean in schools. These analogies and observations led him to frame the question that the model-making of windmills ought to address very differently than others had framed it before.

There is probably more involved than cognitive imagination, though: Dabiri mentions the well-known drawbacks of the kind of windmill considered optimal using the previous approach of looking at the swept area of a single windmill (i.e., the extremely large horizontal-axis wind turbines (HAWTs) mentioned earlier, which need to be spaced far apart in order to achieve the efficiencies for which they were chosen). Some of these drawbacks involve social and environmental impacts, and so have a moral dimension to them. Thus, an openness to, a desire for, even a search for, alternatives to the decision methodology then in use that had given rise to a solution that had such negative effects—not to mention the active use of intelligence and creativity to formulate alternatives—is probably due at least in part to a sensitivity to such moral aspects of design decisions.

What kind of insight was called for in recharacterizing—we might even say, in reframing—the design problem of finding the optimal windmill style and arrangement? Recharacterizing the problem in more general terms is a necessary step, very likely, in that it opens the way to different design possibilities, but, clearly, simply recharacterizing the problem of wind turbine optimization in more general terms was not enough. It merely allowed, but did not show the way, to the solution Dabiri and his colleagues reached. The idea to consider counter-rotating windmills and investigate advantages that might be gained by closer spacing of windmills did not follow from the new problem specification—is there anything more specific we can say about its genesis?

In the windfarm example, I think that what we see in this step is the use of the concept of physically similar systems, albeit in a more qualitative way, during the process of suggesting other solutions. The role of facts in establishing similarity shows up on this qualitative version, too: in order to discern the similarity of two otherwise disparate situations (windfarm and school of fish), one must cognize the situation in a way that the points of similarity become apparent, and this may be a very different way of cognizing it than one would use in simply enjoying the sight of a tropical undersea landscape: different facts are used to establish the similarity discerned. The similarity exists in spite of the fact that there are points of dissimilarity between the two situations that are important when considering other aspects of the problem.[13]

It requires a more encompassing viewpoint, arising from a deeper insight into each of the two situations, to discern that there is a relevant similarity between a turbine vane rotating by the force of the wind impacting upon it, and a fish propelling itself through the water by the force of its tail being swished back and forth, and to recognize precisely what that similarity is. Once one views the two situations in terms of relative motions, the relevant difference between the old way of conceiving the problem (an isolated windmill or, analogously, an isolated fish) and the right way to see the fish situation (in terms of a school of fish, in which the fish do not avoid each others' wakes, but instead take advantage of them by coordinating their movements by swishing their tails in opposing directions), suggests a new sort of configuration of windmills on a wind farm, by the analogy associated with that similarity. The cognitive step of seeing this similarity may be motivated by moral sensitivity and may involve mental imagination and creativity, but the logic used during the model-making is still the logic of physically similar systems. When it comes to computing optimal spacing, a different similarity mapping is called for.

In the published journal article setting out the argument for the feasibility of the new windfarm arrangement (Whittlesey et al., 2010), the kind of behavioral similarity underlying the comparison is noted: "Motivated by the demonstrated benefits of the reversed Kármán vortex street on the propulsion of the schooling fish, we apply the same configuration and similar modeling tools to analyze VAWT [Vertical Axis Wind Turbine] arrays." (ibid, p. 2; Fig. 1 on p. 3). Yet, in selecting the optimum spacing, it is noted that there are

[13] In Whittlesey et al. (2010).

"differences in the criteria of beneficial arrangements. The fish aim to align themselves to optimize their forward propulsion whereas spatial configurations for turbine arrays aim to maximize energy extraction."

I emphasize once again that similarity of systems is always with respect to a behavior of interest, since the point bears emphasis: when establishing similarity with respect to one aspect of behavior, the systems might be similar, but when establishing similarity with respect to another kind of behavior, they might not be similar. We may have a model that is similar with respect to one kind of behavior, and yet have to change the model in order to establish similarity with respect to another kind of behavior—*even for the same target situation being modeled*. There is absolutely nothing contradictory about this. A very simple example of this is that in building a simple physical model of a bridge, one may at first build one that is geometrically similar; in general it will not have the same stiffness as the actual bridge. Any geometrically similar model should suffice for purposes in which geometry is all that matters, such as determining interferences with existing structures, and calculating the surface area requiring painting.[14] To make the model of the bridge similar with respect to bending behavior, however, we would have to *change the model* by making it out of a material that has *different material properties* than the actual bridge (e.g., a material that is more flexible and of a different density).[15] Being able to move between *the use of different similarity mappings between the same systems as required* seems to me to be an essential part of practical reason.

There are a variety of very different approaches to "harvesting" wind energy (more precisely, approaches to transforming the mechanical energy of wind into electrical energy): another quite different approach currently in the prototype and development stage began with the idea of a ladder of kites flown at high altitudes, which would transform the mechanical energy from the wind into rotational energy via the kites pulling on the cables to which they were attached to machinery on the ground. During modeling and prototype testing, the design was modified, and by now has morphed into the idea of a controlled (steered) kite or plane that is used to "harvest" the energy in the wind during flight. (Schmehl, 2012). Other approaches, in contrast, eschew the quest for higher and higher altitudes and instead turn attention to the possibilities of generating electricity from lighter winds.

The point of the windfarm example we have been looking at is that, although the imaginative and creative steps required during the process of model-making cannot be put into a prescription, what we can say is that the logic of analogy, especially the logic of physically similar systems, is still what is relied upon to get things right. Sometimes "getting things right" is a matter of science or engineering. Sometimes "getting things right" is realizing that the characterization of the problem was too specific, or incorrect in some other way. To return to the topic of this paper: does the logic of analogy and similarity of systems apply in matters where values are important; is it relied upon to "get things right" morally or ethically speaking as well as technologically speaking? This is what I now wish to explore.

8. Models of interventions

Since we are interested in if and how the logic of analogy and similar systems plays a role in model-making where values are important, we focus now on models of interventions. Models of interventions are more akin to human action than models of machines are; they involve conceptions of a situation and conceptions of taking action in such a situation.

First, we look at models of intervention aimed at engineering the environment, identify their important features, and judge how they fared. Then, we will ask whether, using the logic of analogy, these models provide us any guidance in making models for engineering the planet (in an attempt to deal with the problem of global warming). A well-known and well-studied example where there are both good and bad models of intervention is the use of pesticides. The use of DDT to successfully eradicate malaria from some parts of the world provides us with a good model of intervention that is detailed and very explicitly spelled out. (This is not to claim that there is a good model for using DDT against malaria in *all* parts of the world.) The use of DDT in failed attempts to deal with the spread of the imported red fire ant in the United States provides an extraordinarily bad model of intervention. At this point in my exposition, I am deliberately being ambiguous about the sense of "good" and "bad."

The year 2012 saw a celebration of the publication of Rachel Carson's *Silent Spring*, which first appeared as a commissioned three-part series in *The New Yorker* in 1962. (Carson, 1962) The background against which she wrote it involved two striking uses of pesticides that could not be more different. The first was the eradication of mosquito-transmitted diseases such as yellow fever and malaria; the second was fire ant eradication.

It may well be that the two uses of pesticides were often spoken of as being similar, in that eradication of a particular species of mosquito identified as the vector of a particular disease was often highlighted in the programs for eradicating yellow fever and malaria. In the case of the malaria program, the pesticide DDT was used to prevent the transmission of the malaria virus at least long enough for the virus to disappear from an isolated population. The model of intervention was very detailed: it included identifying the habits and biology of various species of mosquitoes and identifying which one or ones were responsible for transmitting the virus in that region. The other mosquitoes could be neglected. In fact, it was found that only the female mosquitoes were responsible for transmission. Based on this information about mosquito habits in the areas of Asia in which he worked, the following intervention was designed: A very dilute solution of DDT was sprayed on the interior walls of a residence from which all the eating and cooking utensils had been removed; ingestion was avoided. Another important aspect of the intervention was a criterion for when the spraying could be stopped.

To the credit of the designer of the malaria campaign in the 1950s, Fred L. Soper, his diary reveals that he was constantly inquiring about and pondering over new information about patterns of transmission, traveling to outposts of his campaign and taking note of local differences in the geography, in the political structure of local populations, in evidence of resistance to the pesticides used, and—most importantly for our interests here—constantly evaluating what that information said about his "premise": "that DDT sprayed in the houses will result in the interruption of transmission." (Soper, 1959a; February 28, 1959 entry). The other important premise was that malaria would disappear 'within a reasonable period of time once transmission had been interrupted." (March 19th entry).

The proper goal to focus on was *eradication of malaria*. Although it often did sound as though the goal of malaria eradication programs was *eradication of the mosquito* or a particular species of

[14] Distances in the model would be multiplied by the scaling ratio to obtain the distance in the target, whereas areas in the model would be multiplied by the square of the scaling factor to obtain the corresponding are in the target.

[15] The ratios by which the quantities in the model must be multiplied to yield the values of the quantities in the target are obtained by using the fact that the dimensionless parameters used to establish similarity have the same value in the target as in the model.

mosquito, interest in eradication of the mosquito was involved only insofar as the mosquito was involved in transmission.[16] As time went on, this realization become more and more acute. In 1959 Soper spoke of the "blurring of the immediate objective of the eradication program, viz., the interruption of malaria transmission in each local community for the period required for spontaneous clearing of the infection in the human population...." though his point there was to discourage complacency when a disease was merely still "disappearing", rather than completely eradicated. After the malaria virus was gone, it could not be transmitted, no matter what the mosquito population. That is why he so emphatically emphasized eradication, not merely reduction, of the disease. That 1959 Lecture, "The Epidemiology of a Disappearing Disease: Malaria", ends with a warning: "total victory over malaria can come only as there is total coverage of infected populations and as malaria is not permitted to become a Disappearing Disease before it has been eradicated." (Soper, 1959b)

As with the containment sump design mentioned earlier, this model of intervention depended upon (as Soper called them) "administrative procedures." The malaria eradication was successful in many areas. However, it failed in others. These, he concluded, were due to "administrative failures", not technical ones. He mentions this factor throughout his 1959 Lecture. (Soper, 1959b) That it is a longstanding view, one based on many local experiences, is clear from comments found throughout his diary (e.g., "I find myself giving a lecture on the need for supervision and more supervision until the Director of the Services who must take the responsibility of signing the monthly reports can be certain that the work [ ] at the end of the line, that is in the houses, has been done." (Soper, 1959a, February 18 entry)) A diary entry recording his discussion of record-keeping when setting up a program in Thailand shows where his focus lies: "the unit of interest is the house, as the place where transmission occurs and is the unit of spraying. The house also is fixed geographically and can always be found." (April 11th entry).

So, as long as the premises of his model of intervention designed for the areas covered by the campaign were met, he concluded, the program would succeed in eradicating malaria. However, he was perceptive enough to worry about using the intervention in situations where the assumptions on which the approach was based did not hold, and of the danger of attempting to use models of intervention that might not be applicable to those situations. In his 1959 lecture, he notes the factors that he expected would make the problem of eradication of malaria in Africa 'a formidable one": "the high rate of transmission, the shortage of trained personnel and the difficulty of communications in many parts". He seemed to think the time to attack the problem in Africa had not yet come, and warned "against any attempt to eradicate malaria on too limited a basis" as it could make things worse in the long run, for "reinfection from the periphery may be expected to be a more serious problem there than in other parts of the world." (Soper, 1959b, pp. 364–365) Yet he felt there were in principle no technical factors to preclude malaria eradication in Africa, that his Malaria Eradication Program was still a sound approach, so long as before applying it, the administrators ensured that all the requirements needed for it to be effective, including all the administrative ones, were fully in place. Soper did mention one region of the world where technical problems existed, though: in the Americas, there were serious difficulties with mosquito resistance to DDT in El Salvador, Nicaragua, and some other countries. Near the end of his speech, he indicated that the success of the Malaria Eradication Program showed that the road was open for other "similar action on human, animal, and plant diseases, and insect and plant pests", but the only examples he gave were of diseases.

In parallel with the successful model of intervention using DDT in Asia in the 1950s, was a completely different use of DDT and other, more potent, pesticides in the southeastern United States: the USDA Fire Ant Campaign of the Late 1950s. The model-making activity differed from Soper's in key ways:

(i) This insect, often referred to as "the imported fire ant" itself, rather than any particular effect it caused or disease it transmitted, was viewed as an enemy intruder to be eradicated. In fact, Buhs (2002, p. 388) shows a poster in which the fire ant is depicted using the kind of graphics used in World War II for depicting military battles;
(ii) Rather than being delivered in the light concentrations via a powdered form that Soper used, which is a method that did not harm those humans who came into bodily contact with it (Carson, 1962 I, p. 39), in the Fire Ant Campaign, DDT and other more toxic pesticides were delivered in high concentrations and mixed with fuel oil so as to increase their bioavailability;
(iii) Rather than being delivered indoors and on a household by household basis, the mixture in fuel oil was sprayed outdoors over wide areas (tens of thousands of acres), including private property. Thus, the cooperation of those being affected was not needed in order to carry out the logistics;
(iv) Advice on selecting the suppliers and choosing the concentrations of pesticides to be used was provided by the corporate suppliers of the pesticides, and
(v) The fact that the use of pesticides was likely to result in "flareback" of the insect population on which it was used, as well as other insect populations, was neither recognized nor addressed in the program. (Daniel, 1990)

The Fire Ant program was a failure in multiple ways. Whereas Soper's Malaria Eradication program was effective long-term in many areas, the Fire Ant program was not, and it had devastating effects on other wildlife and the ecosystem. *Fire Ant Eradication* is not a feasible goal, and the proponents of the program had never bothered to try to show that it could be a feasible goal. After the Fire Ant Eradication program, the acreage of fire ant infestation actually increased. (Simberloff, 2007) Nor is eradication of the Fire Ant even desirable: it turned out that, in spite of being imported, fire ants had become a valuable predator of ticks, and ticks transmit disease.[17] More recently, other beneficial consequences

[16] Gladwell's tribute to Soper in *The New Yorker* (Gladwell, 2001) is titled "The Mosquito Killer", but even in discussing the Yellow Fever campaign, Soper was clear about eradication of a certain species of mosquito being only a means to the elimination of a disease; in fact, Soper's important realization after Yellow Fever had reappeared in the absence of the *A. aegypti* mosquito was to emphasize the importance of tracking the disease even where *A. aegypti* was no longer present. (Soper, 1959, p. 360) Soper was well aware of the phenomenon of mosquitoes developing pesticide resistance, too, especially dieldrin; his use of DDT was not insensitive to this fact—he argued against using more of even the very diluted solutions than required and, when recommending its use in India, he mentions as part of the basis for his recommendation that he did not observe DDT resistance had yet developed in the mosquitos there. (Soper, 1959, p. 363) Thus Gladwell's criticism of Rachel Carson, as though her points are a contrast to Soper's, is misdirected. His criticism reflects a common, but inaccurate portrayal of her writings: in actuality, not only did Carson fully recognize, and allow for, appropriate use of DDT in stopping the transmission of malaria (Carson, 1962 I, p. 39), but the indiscriminate use of agricultural spraying that was the main target of Carson's criticism (she urged "selective spraying" rather than "blanket spraying" Carson (1962 I, pp. 98–99)) is the cause of one of the biggest impediments to malaria eradication today: resistance of mosquitoes to DDT. Roberts et al., similarly misrepresent Carson when describing her "crusade against the use of pesticides" (p. xi); they point out something that she was actually keen to urge as well: "Even in the 1950s there was clear evidence that mosquito resistance was evolving from agricultural use of DDT,.... Agricultural use of DDT selected for resistance in mosquitoes because the larvae were present in bodies of water that became contaminated when DDT was sprayed on crops." (Roberts, Tren, with Bate, & Zambone, 2010)

[17] Per ANR-1248, "Imported fire ants can reduce populations of some other pests such as lone star ticks and forage-feeding caterpillar species." (Flanders & Drees, 2004, p. 5)

have become appreciated. (Penn State Cooperative Extension, 2013) The proper goal, it is now recognized, is *Fire Ant Balance*.

Suitable farming practices and integrated pest management, where pesticides are used only sparingly, if ever, and with focused short term goals, is a far better approach even from the standpoint of controlling the spread of the fire ant. (Foil, 2010) The model the USDA program used—indiscriminately spreading a bioavailable toxin over every living thing in a wide geographical region in order to kill as many fire ants as possible—is inappropriate, and the model-making that gave rise to it was poorly done. The only hope of making an appropriate model comes from understanding fire ant activities within an ecosystem that includes wildlife, humans, and farming practices. (Simberloff, 2007) That is what would be analogous to the approach that was used in developing the model of intervention for the Malaria Eradication Program. The difference between the kind of model-making that resulted in an inappropriate model of intervention, and the kind of model-making that resulted in an appropriate one for these two cases of engineering the environment seem to illustrate the points made in Sections 3 through 5 of this paper about model-making in engineering. That is, one must properly identify a whole system that is the subject of the behavior in which one is interested.

These two extremes of programs using DDT and related pesticides were two well-known cases of the use of pesticides already in the social and technological background against which Rachel Carson's *Silent Spring* series was commissioned. There was already outcry against the Fire Ant program, for multiple reasons: power struggles between programs and bureaucracies led to calls for investigations into the program, the harms to wildlife and ecosystems outraged and alarmed conservationists who made their concerns known to Congress and the public, and exposure of intellectual dishonesty on the part of the Agricultural Research Service arm of the USDA about the actual danger posed by the fire ant. (Daniel, 1990; Lear, 1992; Carson, 1962 II, pp. 57–64) Carson was already among the voices challenging the USDA on its claims that DDT and other, more toxic pesticides were not dangerous. She had already collected information on scientific research on such pesticides and was working on a book about it; her charge for the commissioned New Yorker series was to write a piece that reviewed the scientific literature about the effects of DDT and other pesticides and to summarize the dangers in a way that could be understood by the general public. (Lear, 1992) She explained the difference between effects of the DDT powder used in malaria eradication and effects of the DDT mixed with fuel oil and other, more toxic pesticides and then broadcast over vast swaths of land. (Carson, 1962 I, p. 39) She explained how the chemicals used in agricultural spraying of pesticides affected humans via the food chain and how the concentrations were enormously magnified when stored in the fat of animals higher up in the food chain. Her target was the indiscriminate outdoor spraying of large areas of the Southern United States. (Carson, 1962 I, pp. 98–99; II, pp. 52, 74) Using the perspective of the ecosystem of which the fire ant was a part, she cited the scientific research on such indiscriminate spraying, showing both its potential for harm to humans and wildlife, and its hopelessness in achieving the stated goal of the fire ant eradication program. She recognized then what is no longer disputed: due to the combination of the mechanisms of "flareback" after the use of pesticides (Carson, 1962 I, p. 36), and the development of resistance to pesticides, widespread spraying of DDT would actually make the fire ant problem worse. (Carson, 1962 II, pp. 66, 74) Carson's concern was that the USDA was not properly informing the public of the hazards of the program; the purpose was to relay to the public what the scientific research showed. The point of writing the piece was not, contrary to many current claims, to ban DDT (Carson, 1962 III, pp. 66–67; Lear, 1992).

It has been over fifty years now since "Silent Spring" (Carson, 1962) was published. How do things stand now? With respect to DDT, the ban on broadcast spraying of DDT remains, but limited use of DDT indoors (Indoor Residual Spraying), which never went completely out of use, has increased, due in part to the Bill and Melinda Gates Foundation's support and funding for their Malaria Elimination Group (MEG). There are, as Soper predicted, many challenges (in Africa especially), and no single solution will suffice. (Shah, 2010; Nájera, González-Silva, & Alonso, 2011) Insect resistance to the pesticides used is a salient issue (IRAC, 2012). Another point being urged about making models of intervention for malaria elimination illustrates a point made earlier in this paper about the scope of engineering models: the need for "urgent strategic investment into understanding the ecology and evolution of the mosquito vectors that transmit malaria" especially "aspects of the mosquito life cycle beyond the blood feeding processes which directly mediate malaria transmission." (Ferguson et al., 2010)

What lessons can we draw from these examples of good and bad model-making of models of intervention? The challenges in 2012 are different than they were in 1962; some are new, and, ironically, one of them is the subject of a New Yorker article appearing just a few weeks shy of the 50th anniversary of the three-part "Silent Spring" essay. Once again, the intervention involves spraying, but it is at the proposal stage. The article is "The Climate Fixers" (Spector, 2012) and the proposal this time is to engineer not just the environment (ecosystem), but to engineer behavior at the planetary level. The suggestion is to reflect sunlight away from the earth via spraying of aerosols in the atmosphere. The line of thinking is as follows: carbon dioxide and other greenhouse gases already released into the atmosphere will remain for decades, and the change in climate they cause and will continue to cause is dangerous. One quantity tracked to illustrate the change—and the one that climate models are best at predicting—is global mean temperature. One way to reduce global mean temperature is to release aerosols (particles that stay suspended in the air for some length of time) into the air. It is thought that pollution in the past caused some solar radiation to be reflected away from the earth, and that the reduction in pollution has led to the phenomenon of "global brightening." (This is, however, not an uncontested account of the observed data.) It is also known that one effect of the eruption of Mount Pinatubo in 1991 was a reduction in global mean temperature, due to the release of sulfate aerosols. The temperature drop was significant: about one degree Fahrenheit. With this kind of sudden change in solar radiation, one would expect changes in precipitation, too, since evaporative processes upon which the hydrological cycle depends will be lessened. The other problem is that the effect of the sulfate aerosols is short-lived, and when their effect disappears in about a year or so, there is a rapid increase of the opposite sort. Certainly the weather that followed in the US in the two years that followed the eruption of Mt Pinatubo was unusual with respect to precipitation patterns, and, though it is never mentioned in discussions of climate engineering that cite the eruption of Mt Pinatubo as a model for reducing global mean temperature, there did follow a major disaster of epic proportions, one due to unusual precipitation patterns. In fact, when it occurred, it set the record for the most expensive natural disaster in the history of the United States: the Great US Flood of 1993. (Changnon, 1996; Des Moines Register, 1993; Larson, 1996) It lasted for many months, it killed dozens of people, and it permanently displaced thousands. Current methods don't permit reliable prediction of the precipitation patterns that would follow on attempts to engineer the climate by injecting sulfate aerosols into the stratosphere; consequences due to its effects on precipitation are not well understood.

Why the attraction to the eruption of Mt Pinatubo as a model, then? It has to do with the fact that the global mean temperature was in fact reduced, and that the reduction was about as much as the climate models predicted it would drop. But there is another crucial part of the attraction: there is a tendency to think in terms of "radiative forcings" in the design, application, and interpretation of large computer models of global climate, and this encourages thinking of blocking sunlight as 'counteracting' increases of carbon dioxide. (National Research Council, 2005, p. 23)

Just as facts about ecology, geography and the life cycle of the mosquito and the ecosystem were important in good model-making of models of intervention on the environment, so facts about the biosphere and global atmospheric physics will be important here. Some basic facts are: (i) The "greenhouse effect" is crucial to life; it is only because of greenhouse gases that earth has an atmosphere, and all life on earth (the entire biosphere) depends upon it. The existence of a greenhouse effect is not the issue—the issue is a matter of balance. (ii) Sunlight (solar radiation) is responsible for all life and most of the energy transformations (wind, clouds, precipitation) that take place on earth, as well as for carbon uptake in the carbon cycle, since it is photosynthesis that drives the carbon cycle, and the energy powering photosynthesis is sunlight. Sunlight is also, as a consequence, responsible for all nutrition supporting almost all life of every kind on earth. (iii) This proposal is not in any real sense a corrective to the situation. The increase in mean global temperature is due to less longwave radiation being able to pass through the atmosphere back to space than has previously been the case. The proposal would reduce the amount of sunlight available to the biosphere—but the problem of global warming is not a matter of too much sunlight entering the biosphere. The problem is that too much of the energy of solar radiation is ending up as trapped heat.

Let us see if we can use the examples of Soper's model-making of a model for DDT intervention and the example of model-making carried out by the Agricultural Research Service (ARS) arm of the USDA in the fire ant program, to gain some insight on the quality of model-making involved in this proposal. We know that, in his model-making activity, Soper recognized that conditions differed in different countries, and that each new application of his method to a new region had to consider the special conditions there, as well as the time and place of the new implementation. In his model-making, he tried to avoid speculation about the results of the intervention and he avoided epistemic risk-taking. The USDA's model-making activity, carried out without proper regard for the potential for danger, did not. Soper identified the goal of interest (malaria eradication) appropriately and, in each particular application of his method, focused on that, rather than on some other, intermediary goal (e.g., mosquito eradication). The USDA did no such thing; the USDA instead not only inappropriately identified the goal it should pursue (eradicating the fire ant), but chose a method of implementing it that undermined the goal it should have identified (fire ant balance). Finally, Soper obtained advice and funding from sources that had no financial conflicts. In contrast, the USDA obtained funding from Congress and advice from the very sources who stood to receive large financial gains from the use of the products they recommended the USDA purchase using the appropriated taxpayer funds. (Daniel, 1990)

How does the proposal to inject sulfate aerosols into the atmosphere fare in comparison? While its proponents acknowledge, when pressed, that the risks are huge and the effects are not known, it is still being claimed this geoengineering method be viewed as a "Plan B." (Victor, Morgan, Apt, Steinbruner, & Ricke, 2013). The model-making is scant on science, having for support only sketchy anecdote and computational models never intended for such a use. The climate prediction models are, on the account of anyone who is qualified to judge, unsuited for designing interventions.[18] (GAO, 2011) As sophisticated as they are computationally speaking, the models are seriously lacking in many respects[19] important to planning an intervention of this sort, and so will not be informative tools for designing interventions such as this sulfate aerosol project. In fact, aerosols are considered especially poorly captured by existing modeling techniques.[20]

> "Several types of forcings—most notably aerosols, land-use and land-cover change, and modifications to biogeochemistry—impact the climate system in nonradiative ways, in particular by modifying the hydrological cycle and vegetation dynamics. Aerosols exert a forcing on the hydrological cycle by modifying cloud condensation nuclei, ice nuclei, precipitation efficiency, and the ratio between solar direct and diffuse radiation received." (National Research Council, 2005)

Like the USDA's model-making activity in designing its interventions, the model-making for a model of intervention involving the spraying of sulfate aerosols is proceeding in spite of the recognition that there is not, and may never be, sufficient understanding of the complexities involved to properly design a model of intervention. In fact, tests are being urged (by Salam, 2013, who interprets Victor 2013 as a call for small scale tests.)

Another comparison has to do with intellectual honesty. The geoengineering activity being urged is currently presented as a research activity (Victor et al., 2013), just as the USDA's fire ant program was carried out under the arm of the Agricultural Research Service (Daniel, 1990). The background to this is that actually, the current state of things is that a ban on all but small-scale experiments for research studies was agreed to at a UN Convention on Biodiversity, so any real interventions would *have* to pitched as such (Bracmort & Lattanzio, 2013; United Nations Environmental Programme, 2012). The statement of the goal of the research activity is unsettling. Here is why: the statements in the call for experiments involving the actual dispersal of sulfate aerosols are made without appropriate qualifications of the sense of "could": in a recent issue of *Foreign Affairs*, readers are told that "SRM technologies could cool the planet in just a few months by tinkering with the planet's energy balance. [...] The clouds could deflect just enough incoming sunlight to offset, crudely, the number of degrees human emissions have warmed the planet." (Victor et al., 2013) That same article points out that "the science is missing." Why, then, portray the technology as one having such capabilities, when there is no basis for claiming such a capability is possible, even in principle? However the quoted statement was meant when written, the reality is that such a statement exudes a confidence about in-principle attainable knowledge of the consequences of such actions (e.g., "just enough"; "the number of degrees") that is wholly unjustified. The appeals are (at present, at least) only for small-scale experimental tests, which, we are condescendingly told, "should not be seen as the camel's nose under the tent." Unfortunately, seeking permission for small-scale field tests as a path to some (perhaps other) researchers assuming authorization of much larger scale

[18] From Technology Assessment GAO-11-71 under "What GAO Found": "Climate engineering technologies do not now offer a viable response to global climate change. Experts advocating research to develop and evaluate the technologies believe that research on these technologies is urgently needed or would provide an insurance policy against worst case climate scenarios—but caution that the misuse of research could bring new risks. Government reports and the literature suggest that research progress will require not only technology studies but also efforts to improve climate models and data."

[19] "Next Generation of Advanced Climate Models Needed" 7 September 2012. News from the National Academies http://www8.nationalacademies.org/onpinews/newsitem.aspx?RecordID=13430.

[20] Some modeling improvements for reflective aerosols have been developed since the quoted work was published. (Andrews, Forster, Boucher, Bellouin, & Jones, 2010)

spraying is all too reminiscent of the approach taken in the ARS's fire ant program and the propaganda that was used to garner support for it.

The geoengineering scheme for which the so-called "small-scale tests" are supposed to be a test is presented as protection for all from a dangerous hazard, just as the reckless and indiscriminate spraying of pesticides was presented as protection of the entire nation from a national threat (the invading (incoming) fire ant) using military rhetoric. The means used to employ the dispersion of sulfate aerosols (aerial spraying) are such that, inasmuch as any approval will be sought, it will be from a centralized body, not from the individuals affected. Once permission is granted, or some authorization for doing so is justified in some way, no agreement will need to be sought from those affected, and, due to the fact that the means used is aerial dispersal, refusal to participate is not possible. This, too, was the approach taken in the model-making activity for the model of intervention used by the USDA in its broadcasting of pesticides in the Fire Ant Program. The rhetorical ploy of presenting the spraying of something in the skies above, even something toxic, as a public good was used by the USDA in its public relations campaigns, and appears, with little modification, in the almost ubiquitous, sometimes brazenly confident, media portrayals of the potential benefit of this geoengineering scheme (Wilsdon, 2012 is but one of many examples). Recently, one of the scheduled tests was cancelled due to concerns that those researching the scheme stood to benefit commercially from public adoption of the scheme.[21] (Cartwright, 2012) This kind of concern about the proposed geoengineering scheme, too, is reminiscent of the USDA fire ant program (Carson, 1962; Daniel, 1990).

One scientific issue common to the use of DDT and the implementation of the proposed geoengineering scheme deserves special mention: the issue of "flarebacks." Rachel Carson pointed out that indiscriminate broadcasting of potent pesticides could, long term, make the problem worse due to "flarebacks" of insect populations that followed the temporary reduction in their populations effected by the pesticide. (Carson, 1962 I, p. 36; Carson, 1962 II, p. 74) The issue of "flarebacks" is a key problem for this geoengineering scheme, too. For, the earth's oceans, forests, rivers and atmosphere are intimately interrelated and controlled by complicated and subtle feedback mechanisms. Though the use of the phrase "cool the planet in just a few months" without immediate qualification and the innocuous-sounding "tinkering with the planet's energy balance" in the quote above from their article in *Foreign Affairs* rhetorically distracts from this important disadvantage of the scheme, it is not denied by any of the scientifically informed proponents calling for development of the scheme. The fact that implementation of the scheme could make things much worse is admitted, but simply viewed as one of those risks one must take when faced with great hazards. (Spector, 2012) There is no scientifically-grounded expectation that this problem (the climate analogue of the "flarebacks" experienced in the Fire Ant Program), is a tractable problem.

In short, the proposed geoengineering scheme seems a case of model-making that is very like the approach to model-making that gave rise to the irresponsible and harmful USDA Fire Ant program (authorization for which was obtained as a research program), and not at all like the approach employed in Soper's models of intervention, which gave rise to the responsible use of DDT in his highly effective malaria eradication programs, using methods widely regarded as appropriate even today. As in the Fire Ant Program, the public is made to feel that the scheme presented is the only means available; often methods aimed at achieving the same goal by changes in land-use are simply excluded from the scope of a discussion or report on geoengineering. As with the Fire Ant Program, however, other, more appropriate responses to the hazard are in fact readily available. For the threat of global climate change, the list might include:

(i) Forestation with trees that sequester carbon in the soil and provide food, shade, and oil-producing nuts (e.g., drought-resistant, fast-growing moringa trees) and allow for interplanting of crops or interplanting among homes in residential areas; (National Research Council, 2006, p. 246ff)
(ii) A switch from current farming practices to precision agriculture and no-till agriculture, which produce much more vegetation for the same amount of water available and tend to produce significantly fewer emissions of greenhouse gases (e.g., carbon dioxide, methane and nitrous oxide);
(iii) Banning the flaring of natural gas at oil wells, which is an entirely wasteful and useless source of greenhouse gas emissions (Clayton, 2012); and capture of methane emissions from various waste streams;
(iv) The use of reflective poly-covered greenhouses instead of open cropland, as currently practiced in the region around Almeria, Spain, a practice which is thought to be responsible for the decadal trend in reduction of regional temperatures there (Campra, Garcia, Canton, & Palacios-Orueta, 2008);
(v) The installation of green roofs and green spaces to replace energy absorbing asphalt roofs and asphalt lots.

Researchers on geoengineering refer to 10 billion US dollars per year as "a mere pittance" (Victor et al., 2013); the question of what that kind of investment might yield over even a few years if applied to the above alternatives, all of which have additional long-lasting benefits, and some of which even save money and make much better use of environmental resources (e.g., water) in the long term, begs for attention.

Space does not permit a discussion of those alternatives here, however. What is relevant to the topic of this paper is that, like Soper's programs, and like the methods of dealing with fire ants that are now recognized as more appropriate, less harmful, and, importantly, more effective than the ARS's Fire Ant Program (Foil, 2010; Penn State Cooperative Extension, 2013), they are programs implemented on a smaller, decentralized scale. The models of interventions would need to be developed in the course of determining the particular needs for each case.

9. Moral perception and moral particularism

In the opening section of this paper, I said that I would explain that I had come to see *model-making in science* as analogous to a certain account of *moral perception in action*. Whereas moral philosophy is concerned with questions of action or, put more generally, with how one ought to live one's life, our question about models in engineering science here is concerned with questions of how a machine is going to act or, put more generally, with how something in the world works. What's the connection between the two kinds of questions, the one in moral philosophy, and the one in engineering science? In terminology moral philosophers often use: "what a situation requires." McDowell, for instance, in one place writes of virtue as a sensitivity: "an ability to recognize requirements which situations impose on one's behavior. It is a single complex sensitivity of this sort which we are aiming to instill when we aim to inculcate a moral outlook." (McDowell, 1979)

[21] The "planners announced that they have cancelled the test because of concerns that researchers involved in the project could have a commercial interest in its success." (Cartwright, 2012)

Moral particularists are noted for attending to the situational aspects of morality, and most have been concerned to point out the rational and objective aspects of knowing what is required in a situation—to argue, ultimately, that one can make sense of the claim that virtue is knowledge of a certain sort. Yet, according to moral particularism, this ability need not involve the ability to articulate principles about how one should act. (e.g., Dancy, 1983, 2004, 2009, 2010; McDowell, 1979; Wallace, 1996) Now, behavior of machines is not moral behavior, of course. It is not the points about what virtue is that interest us here. It is the points that such moral philosophers have made about an agent's "perception of a situation" that are of interest here, because those points do apply to the human activity of model-making in engineering science. The important thing is "getting it right." For a virtuous agent on the moral particularist account, to "get it right" requires that one's perception of a situation includes what's relevant to revealing what that situation requires of a virtuous agent; put another way, it includes everything that determines how a virtuous agent is required to respond in that situation. For a good model-maker to "get it right" means for the model-maker's perception of a situation to include everything relevant to determining how the machine will respond. (It's an analogy between two different kinds of necessity; lest anyone consider this difference an objection to what I have just said, let me point out that just because there are disanalogies between two things doesn't mean there are not analogies to be drawn between them, too.)

If we are concerned with how a machine will respond, as we are in engineering science, then we need to consider the machine situated in its relation to other things in the world. What those other things are like, and how the machine is situated in relation to them, are relevant to determining how the machine will respond. This is the point made in Section 3 above: "An engineering model is never a model of *just* a machine." Yet, we did not on that account throw up our hands and consider the problem of determining how a machine would respond to be beyond human capabilities. Rather, we noted that engineering science did contain a notion for model-makers to use instead of the notion of a machine that answered to this need: the notion of a system. We discussed the correct manner of delineating system boundaries, so that the system properly accounts for the presence of other things that affect its behavior. If the system is not the same as a machine, though, we run into the question of what else we can draw upon besides physical laws that govern the behavior of a machine.

In Section 4, we noted something that is perhaps unexpected to those who think of engineering as simply more detailed physics: when we asked how far the net must be cast in identifying things that must be specified in order to produce a design specification for a machine to ensure a certain behavior of a machine, we found that the things that must be considered could include things that might be considered subjects for political science: policies governing, and habits of, humans. In Section 5, we then answered the question of what we can use to analyze a system, for we noted that there was an engineering practice that deals with this complexity of systems, the study of physically similar systems. It differs from the approach of using physical laws directly to solve an equation that describes the behavior of interest, but physical laws are involved in that they provide an objective means of saying whether two systems are similar with respect to a certain kind of behavior (whether used in reconstruction of an existing practice of categorizing systems as similar that was learnt independently of knowing these physical laws, or used explicitly in establishing criteria in order to establish similarity of systems and thus categorize systems as similar). I noted that, although the method is designed for use with quantitative characterizations of a system, it might be extended or adapted to qualitative characterizations of a system or situation. We might look to it for a way to understand such things as "*a good x* or *an effective y*, e.g., *a good response to an opportunity* or *an effective means of stimulating growth*" in a way that does not require formulating a principle or criterion of "good" or "effective", but, rather, in terms of being able to say when two situations are similar with respect to that qualitative feature. (This is on analogy to using the methodology of physically similar systems to say if and when two systems are similar with respect to a certain kind of behavior.) Though I did not mention it in Section 5, the point about establishing similarity of systems might be seen on analogy with some points Dancy has made about the particularity of relevance.[22]

I then presented the example of how the problem of harvesting energy from the wind was reconceived during the process of modeling machines for doing so via perceiving similarities between biological situations (fish swimming in schools) and an envisioned windfarm, in Section 6. Here we saw a sort of symmetry with the moral perception case (or, for those who understand what duality in mathematics, a sort of dual to it). That is, whereas moral particularists have been concerned to show the knowledge involved in moral perception, in that facts are involved in such perception, here I am pointing out the sensitivity to moral aspects of a situation involved in model-making in engineering science. The sensitivity to the moral aspects of consequences of using different windmill designs likely did play some part in the creative process described in Sections 6 and 7 that resulted in coming up with the re-envisioned problem statement for windfarm design.

In Section 8, I pointed out that many technical issues involve not just models of what is in the world that engineering science methods are meant to deal with (machines, organisms, natural processes) but models of interventions. These involve the human activity of making a model, as before, but the models more explicitly highlight the actions of humans. We compared two examples of human interventions on the environment using DDT, one of which is now seen as bad, or inappropriate, the other which is regarded as good, and appropriate. I suggested, and showed how, we might use an approach analogous to the method of similarity of systems, to judge a current controversial proposal. Yet, this reasoning does not have quite the same status as in the engineering science case, i.e., the status of an inductive inference. What status does it have, then? Here I think the moral particularist's view is compatible with the methods I've discussed here: that, in giving reasons for acting or judging something to be good, one is pointing out features of a situation[23] that help explain why that situation is a good or desirable situation.

In pointing out that one situation is similar to another, we are regarding one situation as a model of the other. When the subject area is physical science, that is the methodology of physically similar systems; I have been concerned to show that we use scientific theories to establish such similarity between systems. In pointing

[22] Dancy makes the point (Dancy, 1983, p. 534) that the notion of general relevance of such a feature does not make sense; only "relevance in a particular case" does. This is very like how relevance works in discussing physically similar systems; we point out a fact because it is relevant to showing that the behavior of two different systems is the same, and it is only in the context of an entire system and a specified behavior of interest that a judgment of relevance would even make sense. The points made about lessons learned from comparing cases are related to this point, in that the value of making such comparisons is not to discover or cite a principle that covers both cases, but to see each of the two cases with more insight.

[23] It is important to Dancy's view that features do not in and of themselves contribute to goodness or badness: "Features have, as we might put it, variable relevance. Whether a feature is relevant or not in a new case, and if so what exact role it is playing there (the 'form' that its relevance takes there) will be sensitive to other features of the case. This claim emerges as the consequence of the core particularist doctrine, which we can call the holism of reasons. This is the doctrine that what is a reason in one case may be no reason at all in another, or even a reason on the other side." (Dancy, 2009)

out that a situation is similar to another with respect to the ways that are relevant to it being good or desirable, we may be expanding from the subject matter of physical relations and physical theories, but, it seems to me, we are doing so by using an extended notion of the method of physically similar systems.

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